PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. IV. 1860–61. No. 53.

SEVENTY-EIGHTH SESSION.

Monday, 26th November 1860.

Dr CHRISTISON, V. P., in the Chair.

The following Council were elected :—

President.

HIS GRACE THE DUKE OF ARGYLL, K.T.

Vice-Presidents.

Sir DAVID BREWSTER, K.H.
Dr CHRISTISON.
Professor KELLAND.
Hon. Lord NEAVES.
The Very Rev. Dean RAMSAY.
Principal FORBES.

General Secretary,—Dr JOHN HUTTON BALFOUR.

Secretaries to the Ordinary Meetings,—Dr PLAYFAIR, Dr ALLMAN.

Treasurer,—J. T. GIBSON-CRAIG, Esq.

Curator of Library and Museum,—Dr DOUGLAS MACLAGAN.

Councillors.

ANDREW MURRAY, Esq.
Rev. Dr LEE.
D. MILNE HOME, Esq.
Professor C. INNES.
Dr LOWE.
Professor W. J. M. RANKINE.
JAMES DALMAHOY, Esq.
Dr JOHN BROWN.
Professor FRASER.
JAMES LESLIE, Esq. C.E.
Dr SCHMITZ.
Dr SELLER.

Monday, 3d December 1860.

His Grace the DUKE OF ARGYLL, President, delivered the following Opening Address:—

One of the duties which devolve upon me to-night, and one with which it is perhaps best that I should begin, is the melancholy duty of recording the names of those whom death has separated from our fellowship during the Session 1859–60. They are as follows:—

William Alexander, Esq.	Sir James Forrest, Bart.
Dr James Andrew.	Sir John Hall, Bart.
Rt. Hon. Lord Arbuthnot.	John Lizars, Esq.
Sir T. M. Brisbane, Bart.	Sir John Melville.
Dr George Buist.	Dr George Wilson.
Hon. Mountstuart Elphinstone.	

The Fellows elected in Session 1859–60 are eight,—

Dr William Robertson.	George A. Jamieson, Esq.
Dr Frederick Guthrie.	Rev. Leonard S. Orde.
J. Alfred Wanklyn, Esq.	Patrick Dudgeon, Esq.
Professor MacDougall.	William Chambers, Esq.

Total Number of Fellows for 1859, . . 256
„ „ 1860, . . 253

I grieve to add, that the last twenty-four hours has added to the list of deaths the distinguished name of the Rev. Dr James Robertson, Professor of Church History in the University of Edinburgh.

As I rejoice to hear that a detailed memoir of our late President is to be communicated to the Society at a later period, I shall not feel it incumbent on me to do more than trace the main outlines of his public career.

Sir T. BRISBANE was descended from an ancient and honourable family, whose representative occupied the high place of Chancellor of Scotland in the middle of the fourteenth century. He was born in 1773, and entered the army in 1789. A contemporary of Arthur Wellesley, he was early thrown into his society in Ireland;

and thus began a friendship which was cemented by a close companionship in arms, and lasted to the end of the great captain's life. Sir Thomas Brisbane's active military service began in 1793, in which year his regiment formed part of the Duke of York's expedition to Holland. From 1795 to 1798 he was engaged in the various affairs by which the West India Islands were successively reduced. It was during his voyage out in 1795, that having been in imminent danger of shipwreck in a collier transport, from the ignorance of the captain, he was first led to direct his attention to astronomical observation. "Reflecting," he says, "that I might often, even in the course of my life and services, be exposed to similar errors, I was determined to make myself acquainted with navigation and nautical astronomy; and for that purpose I got the best books and instruments, and in time became so well acquainted with those sciences, that, when I was returning home, I was enabled to work the ship's way: and having since crossed the tropics eleven times, and circumnavigated the globe, I have found the greatest possible advantage from my knowledge of lunar observation and calculations of the longitude."

Having acquired by purchase in 1799 the lieutenant-colonelcy of the 69th regiment, he returned to England, but finding that that regiment had meanwhile been sent to Jamaica, he was obliged to repair to that island in the following year. The corps of which Sir Thomas thus took the command appears to have been brought by carelessness and inefficiency on the part of its previous commanders into a very disorganised condition, from which it was speedily redeemed under the management of Brisbane, and this so effectually as to secure for him the highest encomiums of Sir George Nugent, then governor of Jamaica.

Sir Thomas Brisbane's health having suffered severely from the effects of climate, he was obliged to retire on half-pay, when in 1804 his regiment was ordered to India. But in 1810 he was appointed to the staff at Canterbury as assistant adjutant-general; and on the army going out to Portugal, he applied for an appointment under his old friend Sir Arthur Wellesley. In 1812 he secured this great object of his ambition, and as brigadier-general joined the head-quarters of the army then at Coimbra. "The Duke," says Sir Thomas, "received me with the utmost kindness, and said he was glad to see me, as he had two brigades vacant for me."

Sir Thomas remained in this high command throughout the remainder of the Peninsular war, and for his distinguished services, especially at the battle of Orthes on the 27th February 1814, he had the honour of receiving by name the thanks of the British Parliament.

At the close of the Peninsular war, Sir Thomas Brisbane was selected for the command of one of the brigades which were then sent out to Canada, where he used his influence in putting an end to the barbarous practices too often resorted to by both parties in the unfortunate war with America. The escape of Napoleon from Elba recalled Sir Thomas in haste to Europe, where, however, he arrived too late to take part in the final triumph of his great Commander. It is interesting to read Sir Thomas's account of the Duke's language when twelve fine fresh regiments of his best and oldest troops joined him at Paris. Looking down the lines of nearly 5000 men each, the Duke exclaimed, "Had I had these men at Waterloo, I should not have wanted the assistance of Prussians."

There is one curious incident of this period of Sir T. Brisbane's life which is specially interesting to us. It is well known how intense was the feeling of bitterness against the French government and people roused in the German nations by the cruel humiliation they had all successively undergone from the successful tyranny of Napoleon. Some of the public buildings of Paris, commemorative of his victories, were saved only by the personal interference of the Duke of Wellington. It appears that another, the abode of no less celebrated a body than the Institute of France, was saved through the appropriate agency of Sir T. Brisbane. The claim thus established on the favour of the most distinguished scientific society in the world, in addition to that founded on his own acquirements and pursuits, was speedily acknowledged. On the motion of Bouvard, the French astronomer, Sir Thomas Brisbane's name was added, by an unanimous vote, to that roll of membership, which affords, and has long afforded, one of the most valued honours attainable by the successful cultivators of science. It is impossible not to be reminded by this circumstance of that other not dissimilar reward which Milton, in the proud consciousness of his own immortal powers, promises to him who should defend and spare his house from the dangers of a captured city :—

"Captain, or Colonel, or Knight in arms,
Whose chance on these defenceless doors may seize,
If deed of honour did thee ever please,
Guard them; and him within protect from harms.
He can requite thee, for he knows the charms
That call fame on such gentle acts as these;
And he can spread thy name o'er lands and seas,
Whatever clime the sun's bright circle warms."

No better tribute can be rendered to the military character and abilities of Sir T. Brisbane, whose active professional career closed with the end of the war in 1815, than that rendered by the Duke of Wellington in an answer which is recorded by our late President himself. "On my return from America, the late Major-General Sir Manby Power, and the late Lord Kean, informed me that they had written to the Duke of Wellington at Brussels, offering themselves for employment in the army which he was forming for Waterloo. His grace replied that he should be happy to comply with their request, but he could hold out no promise to them until Sir T. Brisbane had received the division which he preferred. This I learned from the above-named generals, but the duke never mentioned it to me himself."

In 1820 the continued favour of his old commander procured for him the governorship of the important colony of New South Wales. It was this command at the Antipodes which enabled Sir Thomas to render to astronomical science those new and important services which procured for him, four years after his return, the gold medal of the Royal Astronomical Society of London. He established, and maintained entirely at his own expense, the now celebrated observatory at Paramatta. So early as 1808, when his health had compelled him to retire for a time from active service, he had erected an observatory at Brisbane, his native place; and some of the instruments procured for this establishment were the first with which observations were begun at the Antipodes.

I am indebted to my friend Principal Forbes for an interesting note on that portion of Sir T. Brisbane's life which bears most closely on his connection with this Society.

"Sir T. Brisbane was elected an F.R.S.E. in 1811, but in consequence of his various military appointments abroad, he did not personally take much part in its proceedings until about 1826, when his name appears on the list of the Council. He had, how-

ever, manifested his warm interest in the Society by the contribution of several papers connected with his favourite subjects of Astronomy and Meteorology. In 1832 he succeeded Sir W. Scott as President of the Society, an honour which he fully appreciated to the very last. While his health remained tolerably good, he took a very active and warm interest in the proceedings of the Society, and to his considerable personal inconvenience he, for many years, came by coach from Kelso to Edinburgh, on the first Monday of every month during the winter, when he attended the Club dinner, and afterwards presided at the evening Meeting.

"A certain simplicity of character, combined with a dignity and courtesy which peculiarly became him, made him deservedly and universally popular among the fellows. The perfect disinterestedness with which he devoted himself to science, added to this favourable impression a feeling of sincere respect. He was lavish of money when any scientific object was in view. Many an unfriended but ingenious person has been encouraged by his liberality, which only erred sometimes on the side of being too indiscriminate.

"Sir Thomas may be said to have spent, not one, but several fortunes in the cause of science; and all the while his personal habits were of the most simple and unpretending kind. About seventeen years ago, having fallen heir to a considerable property, his first thought was how to spend it best for the advancement of his favourite sciences. After consultation with one or two persons on whose judgment he relied, he determined on erecting the magnetical and meteorological observatory at Makerstoun, and on supporting the needful staff of observers at his own expense.

"The valuable observations which were made there, most ably superintended, for the most part, by Mr J. Allan Brown, were afterwards printed at great length in the Transactions of the Society, at the joint expense of the Society and of Sir Thomas himself. The value of these records—extending to three thick quarto volumes—will be hereafter even more appreciated than they are at present. They form probably the greatest contribution made to science by Sir T. Brisbane; hardly even excepting the establishment of the Australian Observatory. They have a double interest for us, as being a unique contribution to the science of his native country: and he was liberally anxious that the Royal Society should be so far associated with him in this truly patriotic work.

"The Society has striven to show how much they appreciated the zeal of Sir T. Brisbane in this matter by taking the initiative in providing for the publication of the final, but less continuous, observations, both magnetical and meteorological, which were made at Makerstoun subsequently to the year 1846, with which the records contained in Vol. XIX. of our Transactions terminates.

"Sir Thomas, as usual, entered warmly into the scheme, and defrayed an equal share of the expense. A great part of the proof sheets were put into his hands not long before his death; and it will be with a melancholy satisfaction that the fellows will receive, on this first anniversary meeting since his death, the fasciculus containing the last bequest to science of our late eminent and disinterested President.

"It would be unjust to Sir Thomas Brisbane's memory not to add, that when from increasing weakness and disease he became wholly incapable of attending the meetings, he, not once, but repeatedly, placed his resignation in the hands of the Council. But they, acting as just interpreters of the feeling of the Society on each of those occasions, besought the veteran general to remain at their head, confident that, in heart at least, he was as devoted as ever to the cause in which they, as well as he, had laboured."

For myself, I must express my great regret that I have never had the honour of knowing, or even seeing Sir Thomas Brisbane, and that therefore I have no means of speaking, except on the authority of others, of those personal qualities which are alluded to by Principal Forbes in the passage I have now read. But from other sources I know enough of the incidents and tenor of his life to entitle me to say that, eminent as Sir T. Brisbane was as a soldier and as a man of science, he was not less remarkable for the benevolence of his heart, and the highest virtues of the Christian character.

Among the Fellows of this Society whom we have lost during the present year, there is another whose name I cannot pass by in silence, or with mere mention only, I mean the name of Mountstuart Elphinstone. In all probability there are few members of this Society now present to whom this distinguished man was personally known: because the greater part of his life was spent in India, and the remainder of it in very close retirement. But his name is familiar to all of us as one of the most eminent among those

whose courage and ability have built up the colossal fabric of our Indian empire. So far as active service is concerned, he was a yet earlier companion in arms of the great Duke than Sir Thomas Brisbane. Alternately acting as soldier and civilian, as in the earlier days of the "Company" all her great servants occasionally did, he took an active part in the campaign which founded the fame of Arthur Wellesley, and, to use the striking words of Lord Ellenborough on a late occasion, "He saw on the field of Assaye the promise of the field of Waterloo."

Mountstuart Elphinstone has, however, a higher claim on the grateful recollection of his country. When war had done its work, and the time had come for governing the people who had been conquered, his powers of administration were as conspicuous as his courage in the field. By the universal consent of all who know the history of our Indian empire, he is regarded as one of the very greatest of those whose wisdom and virtue have tended to reconcile its people to British rule, and have founded those traditions of government which, modified more or less by the progress of events, must continue in the main to be the guide, not only of us in India, but of all nations who undertake the difficult and responsible duty of ruling other nations, different from themselves in race, language, and religion.

George Buist, LL.D., F.R.S.S. L. and E., and G.S., another recently deceased fellow of the Society, was born at Tannadice in the year 1805. His father having been minister of that parish, which is in the presbytery of Forfar, Dr Buist was educated at St Andrews, and studied divinity for the purpose of becoming a minister of the Church of Scotland; but, though licensed to be a preacher, he never was ordained as a minister of the Church. He cultivated with assiduity the study of science, especially in its bearings on natural history and geology, founded a provincial society for its prosecution, and gained the prize offered by the Highland and Agricultural Society for an account of the Geology of Perthshire, which is published in the Transactions of that Society. During his residence in India he contributed many papers of interest to the scientific societies of that country. In addition to these, he also published papers of interest on its antiquities and history. Many important public works enjoyed much benefit from his active co-operation. Among these may be mentioned the establishment at Bombay of an

industrial school for natives, wherein a knowledge of British manufactures was taught, and which led the way to similar industrial institutions for the other presidencies. His energy and abilities were appreciated by the Indian Government, notwithstanding that he was in frequent political opposition to it; and when the superintendence of the government printing-press and government *Gazette* at Allahabad became vacant, Dr Buist was appointed to it by Lord Canning. Dr Buist died on a voyage to Calcutta, on the 1st day of October last.

I wish I were capable of presenting to the Society anything like a really useful review of the progress of science during the year which is about to close. This I cannot pretend to do; but perhaps I may be allowed to direct your attention to one or two subjects to which that progress has been important.

To begin with our own country, and with an investigation the importance and interest of which has been acknowledged by the Society in the grant of the Brisbane medal,—I have reason to believe that Sir Roderick Murchison has been prosecuting with farther success his examination and reclassification of the more ancient rocks of Scotland. The clue afforded some years ago by the discovery of Mr Peach, that the limestones of Duirness in Sutherland contained fossils of the Lower Silurian age, has been followed up by our distinguished countryman Sir Roderick, with his usual indefatigable perseverance, and his usual sagacity of interpretation. The result of his last researches goes far to extend the light already thrown on the rocks of Sutherland and Ross to the vast series of micaceous and quartzose strata which constitute the great bulk of the Western Highlands in the counties of Argyll and Inverness. And I think it a circumstance worthy of mention, that some years before the discovery of the Sutherland fossils, and before, therefore, any clue from organic remains had been afforded, Sir Roderick Murchison had suspected that the whole series of metamorphic slates in the district to which I refer were nothing more nor less than altered strata of Silurian age. He expressed that suspicion strongly to myself in 1850, when I had an opportunity of pointing out to him some of the more characteristic beds in the neighbourhood of Inveraray. During this last summer and autumn, he has traced the upward series of rocks from what he calls the fundamental gneiss

in Sutherland and Lewis, southward to the islands of Islay and Jura, and by a close examination of the stratigraphical relations, is now prepared to furnish proof of the truth of the conclusion to which by a species of instinct he had been led before. In one of the facts upon which this determination rests, I think I can venture, from personal observation, to confirm his argument. The term gneiss had been correctly applied by M'Culloch to the fundamental rock of the outer Hebrides, a rock which reappears in great mass on the south-west coast of Sutherland. But unfortunately he applied the same term to other rocks, which are now proved to overlie beds containing Lower Silurian fossils. He thus confounded strata which are separated by immense ages from each other. Now, Sir Roderick Murchison has pointed out the essential differences of lithological character which distinguish the fundamental gneiss from all the rocks of the overlying series. When these differences are once pointed out, it is impossible to mistake the two. The fundamental gneiss is distinguished by the predominance of hornblende, so thickly laid, generally in lines parallel to the stratification, as frequently to render the stone almost black. The felspar and mica are generally found in large separate crystals and plates; and it is not unfrequently intersected by veins and masses in which the same mineral constituents are more perfectly mixed in the form of granite.

To this rock, which is largely developed in our North American possessions, where also it is succeeded by a very similar series of overlying deposits, the term "Lawrentian" has been applied by Sir William Logan.

This term Sir Roderick Murchison proposes to retain for the oldest stratified rock yet known in the world. Upon this fundamental Lawrentian gneiss are piled the vast series of Cambrian strata which constitute the great mass, and sometimes the whole, of the most striking mountain-forms on the west coast of Sutherland and Ross. These strata are estimated by Murchison to measure some ten or twelve thousand feet in thickness. Resting again unconformably upon these Cambrian beds, and capping with their white quartzites many of the mountains, the true Silurian rocks appear, distinguished—mainly in the limestone bands, but also, though more rarely, in the quartzites—by orthoceratites, and other characteristic fossils. Intercalated among these, and therefore having their relative age clearly determined, occur those other more crystalline and

metamorphic strata to which the same term gneiss had been also unfortunately applied. But no two rocks can be more different than those overlying rocks from the fundamental gneiss. I have never seen in any part of the South-west Highlands, among the mountains which M'Culloch assigns to gneiss, any rock approaching in character to the gneiss of the Hebrides and of the north. The question, however, will, I have every reason to believe, be finally settled by the proofs which are about to be brought before the Geological Society. Sir R. Murchison has found that the islands of Islay and Jura present perfect repetitions of the phenomena of Sutherland, and that the quartz rocks and limestones of Silurian age are superposed conformably and without a break by the micaceous and chloritic series which occupies such large tracts on the opposite mainland, and which, folding over a little south of Loch Tay, and clasping round Schiehallion, again rises up to the north of Loch Rannoch, and allows the lower quartzites and limestones to reappear. Very curious questions arise as to the causes of the metamorphic action which has so completely altered the structure of beds lying over others which remain comparatively unaffected. Some geologists have been inclined to deny the existence of true stratification in the micaceous chloritic schists of the South-west Highlands, and to assign the appearances to lamination or slaty cleavage. I must say I agree entirely in the view taken by Sir R. Murchison, that this doctrine is wholly untenable. Indeed, I can with difficulty suppose its being held by any one who is familiar with the districts in which these rocks prevail. It may safely be affirmed that there is no one indication or feature of true aqueous stratification which is awanting, except the presence of organic remains. There are the same alternations of siliceous, muddy, and calcareous beds, which everywhere characterise a long continuance of marine deposit thrown down under various mineral conditions.

It is well to observe that this new classification of the rocks in the north-west of Scotland adds additional force to an argument long ago used by Sir Roderick Murchison in reference to the bearing of geological evidence on the great question of the beginning and succession of life. The Silurian strata, in which fossils have been discovered, are more crystalline and more highly metamorphic than the Cambrian strata which lie below them. Yet, in Scotland at least, no organic remains whatever have as yet been discovered

throughout the vast series of beds which belong to those old deposits; whilst elsewhere the few forms of life hitherto discovered indicate what M. Barrande has called a "Primordial Zone." These successive formations have now been traced, and more or less examined, in almost every region of the globe, and everywhere the same limited assemblage of organic remains has been established—the same total absence of any indication of terrestrial life—the same few generic types, chiefly of crustacea, cephalopoda, brachiopoda, most of which have long since ceased to be, whilst one at least has survived every subsequent revolution, and is still living in the present day. On the other hand, it will no doubt be argued by those who take an opposite view, that the circumstances attending this reclassification of the older rocks of Scotland tend more than ever to teach the necessity of caution in the interpretation of negative evidence. The abundant existence, it will be said, of organic life during the ages of the Silurian deposit is beyond question. Yet all traces of it have been obliterated absolutely throughout a vast series of beds: in others, the indications are so exceedingly obscure that their character is altogether doubtful; whilst only in one or two thin seams of limestone, and in still rarer quartzite beds, has an unequivocal record been preserved of the highly organised and abundant molluscan life of the Silurian seas.

Before passing from the Geology of Scotland, I must direct the attention of the Society to the very beautiful geological map of this city and its vicinity which has been lately published by the Department over which Sir R. Murchison presides as Director-General. The coal-basin, with its coal crops and faults, was the work of Mr Howel; the rest was surveyed by Mr Geikie; both these gentlemen being geologists of the Government Survey. The admirable care and exactness with which they have given the minutest details of a very varied and intricate district, is an excellent example of the high economical as well as scientific value we may anticipate of the geological survey of the country.

The oldest formation in this sheet is the *Lower Silurian*, of which two small patches occur along the southern edge of the map. They belong to the great Silurian tract of southern Scotland, against which the upper Old Red Sandstone and carboniferous rocks of the Lothians rest unconformably. There are at present known only two areas of *Upper Silurian* strata in Scotland, of which one occurs in the Pentland Hills, and is mapped in the present sheet. It con-

sists of highly inclined shales and sandstone. Mr Charles Maclaren was the first to detect organic remains in these strata. About twenty-five years ago he found two orthoceratites, but in a fragmentary state. In the year 1857, when the Geological Survey extended into the district in question, Mr Geikie first made known the richly fossiliferous character of these Silurian strata, the assemblage of fossils unequivocally indicating the horizon of the Ludlow rocks of England. On the edges of the upper Silurian beds rest unconformably the upper Old Red Sandstones and conglomerates, with enormous interbedded sheets of felstone, which form the chain of the Pentland Hills.

The great Lower Carboniferous group is well shown in the area embraced by the present map. It occupies the whole of the district between the Bathgate hills and the Pentlands, and contains in that region a seam of limestone, which is the equivalent of the Burdiehouse limestone on the east side of the Pentland ridge, and also a seam of coal that appears to be quite local. The line of outcrop of these two seams, as traced on the map, will show the intricate character of the geological details. Perhaps the most remarkable feature in the Lower Carboniferous series of the Lothians is the abundance and variety of its associated contemporaneous igneous rocks. There is no well marked zone in the series which does not at some locality in this region display its sheets of greenstone, felstone, or ash.

The Carboniferous limestone of this sheet shows characteristically the Scottish type of that sub-formation. Its base consists of limestone bands, with associated shales, sandstone, and coals. Above these comes the group of coal-bearing strata, known as the "Edge coals" of Midlothian. But these are not the Coal-measures of England, seeing that above them there are bands of limestone, with true Carboniferous limestone fossils. The Millstone grit has not yet been satisfactorily determined, but its place may be represented by some of the thick sandstones of Roslin.

The Coal-measures proper, or "Flat coals" of Midlothian, occupy the centre of the Edinburgh coal-basin. They are truly the equivalents in position as well as fossils of the Coal-measures of England.

It is deserving of remark, that while, in the Lower Carboniferous strata and in the "Edge-coals" of Linlithgowshire, volcanic rocks

abound, none occur in the Edinburgh coal-field, although they were abundant in that district during the earlier part of the Carboniferous period.

The system of parallel faulting of the Pentland Hills is also worthy of notice, as accounting for the small development of Lower Carboniferous strata on the east of the chain, and their great expansion to the west. The highly inclined character of these strata along the east side of the hills (some being quite on edge, hence called "Edge-coals"), arises from the downthrow of the whole coal-field against the older rocks of the chain. A detailed description of the sheet from the geologists before named is at present in the press.

The attention, not of geologists only, but of men of science in several departments, has, during this and the preceding year, been fully awakened to the importance of a discovery which is really of much older date—viz., that flint implements, the work of man, are found in beds of drift gravel associated with the bones of the last generation of the great extinct mammalia. The full significance of this fact is only now being fully recognised, and many of the conclusions which it may tend to establish are subject to much doubt, and will probably form the subject of increasing controversy. But it is only necessary to have a clear idea of the facts as they have been now ascertained, to see that one conclusion at least is placed beyond all question—viz., that great physical changes on the surface of the earth, and these, in part at least, effected by the agency of water, have taken place since the creation of man.

Whether this conclusion carries the creation of man farther back than had commonly been supposed, or whether it merely brings nearer to us than we had before conceived, the last great changes which have produced the existing surface, is the main question on which debate arises. As geology gives no certain data for computing positive, but only relative time, this question is necessarily involved in much obscurity. But there are certain limits within which, after all, the controversy is confined. It is well to observe that, according to the principle on which geological times and epochs are classified, the human epoch remains, after these discoveries, very much where it stood before. It is true that many of the large animals, with which the traces of man seem to be connected, are now extinct; but a very much larger number are still living. The

Molluscan Fauna, which plays so important a part in ages of geologic time, is absolutely the same. The general aspect of animal life is the present aspect, with the exception that a certain number of species of the larger Herbivora and Carnivora have become extinct. But such extinctions, local in many instances, and total in some, have taken place in historic times, and are in visible process of accomplishment even now. Such extinctions do not constitute a new Fauna, nor, according to the received principle of classifying past times, do they mark a new geological age. The era of man, therefore, remains, geologically speaking, in the same relative place in which it stood before—the very last and latest of the world.

But the fact that human implements are found under great beds of gravel and of earth formed by water, whether of rivers or of the sea, at an elevation which in either case would imply changes of level, such as, if general, would be enough to revolutionize the whole aspect of our now habitable surface, is a fact which casts new and important light on the (geologically speaking) very recent date at which those changes have taken place.

Whether the men who formed the implements were or were not contemporary with the living quadrupeds whose bones are associated with these implements, seems to me a subordinate question. The mere fact of such association may not absolutely prove the point, because it is conceivable that the bones may have been merely re-aggregated from an older fossiliferous deposit. But I suspect that the reluctance to admit the contemporaneity of man with those animals results from the reluctance to admit man's priority to such physical changes as are supposed to separate us from a Fauna typified by the Mammoth and the Elk. If, therefore, the fact of such priority be proved from the stratigraphical position of the flint relics, wholly independent of any argument derived from organic remains, the importance of the question respecting the human age of the great mammals will be much diminished. It may be well, therefore, to keep our attention firmly fixed on what is the really important question—the nature and position of the strata in which, and under which, the flint implements have been interred. Going no farther for light upon this question than the particular beds at Amiens and Abbeville in France, where the implements have been found in greatest abundance, it is enough to record the facts. The flints are embedded in a stratum of gravel, which rests directly on an

eroded surface of the chalk, and contains along with the hatchets the bones of the great extinct mammalia. This is again surmounted by a bed of sand from seven to ten feet thick, in which only a few rare bones and implements have been found. This is again capped by a second bed of gravel from two to five feet thick; and lastly, on the top of all, is a bed of brick-earth, in which, as if to afford the very poetry of illustration, are to be seen the tombs of Roman Gaul. Such is the position of the beds with reference to each other. But what is their position with reference, not to each other, but to the surrounding country? The gravel-bed extends to points upwards of a hundred feet above the level of the river Somme, which occupies the bottom of the existing valley. It is described by Professor Rogers, a most competent and accurate observer, as extending to the summits of the plateaux which determine the existing drainage. Whether, therefore, the water which formed those beds were marine or fluviatile, in either case such changes of level are implied as would be sufficient, if general, to alter widely the existing distribution of land and sea.

Here, then, the question arises, Were those changes local—confined perhaps to the district of Western France? Connected with this question, another immediately occurs: Is not this bed of gravel identical in character and composition with similar deposits in other countries? Is there anything to distinguish it from the gravels containing precisely the same mammalian bones which are familiar to geologists in almost every country, and which have been recognised every here and there over the whole of Europe, from Siberia to Palermo, and from the basin of the Thames to the valley of the Danube? So far as I have been able to gather from the papers which have detailed the facts, there is nothing to indicate any difference whatever, except that, at least until this discussion arose, human implements had nowhere else been recognised as associated with the drift. The absence of such remains elsewhere, however, would go for little in establishing a difference, because it is clear that the men who existed before the formation of the Abbeville beds were rude, and probably widely scattered savages, distant outliers of their race. The chances, therefore, were infinite against the preservation either of them or of their works. But even this distinction, it would appear, is being broken down. It is now recollected that so long as sixty years ago, human implements had been discovered in Suffolk

under similar conditions, and the fact communicated to the public in an archæological journal by the discoverer Mr Frere. The spot has been since visited by Mr Prestwich, fresh from the Abbeville beds, and he recognises the same phenomena. But this is not all. The scent, once taken up, is becoming stronger and stronger, every day. Closely connected with the period of the drift-gravels are the ossiferous caves and caverns so common all over Europe where limestones prevail. They have been long known to contain a profusion of bones of the extinct as well as of living mammalia. Here, again, it is now confidently asserted that human implements are being found under conditions which leave no doubt that, whether man was or was not contemporary with these animals, he must at least have preceded the action of those agencies which brought the bones together. The evidence in this case must necessarily be more liable to erroneous interpretation than in the case of implements found in undisturbed beds of gravel, because caverns must at all times have been a resort of savage tribes whenever the entrances were accessible from the surface. But the evidence seems to be such as is sufficient to convince examiners so careful and acute as Dr Falconer and Mr Prestwich of the undoubted priority of man to that diluvial action which appears to have swept into those caverns their mixed contents. But this is not all. It is now recalled to mind, that so long ago as 1833, a M. Schmerling had published Researches into the Ossiferous Caverns of Belgium, in which, not implements of man only, but his teeth and his bones, and portions of his skull, had been found so thoroughly mixed up with the remains of the lower mammalia, as to leave in his mind no doubt, if not of their contemporaneous life, at least of their contemporaneous entombment in the spots where they are now found. These are remarkable facts; and in so far as they indicate that the phenomena of Abbeville are closely related to others observed in many different parts of Europe, they go far to prove that the French gravel-beds were due to no mere local cause, but to some diluvial action which was general, and therefore in all probability due in great part to the waters of the sea.

I need not point out how many and how interesting are the questions which this discovery raises in our minds. Was this incursion of the waters of the sea, over a pre-existing land, sudden and transient, or gradual, and of long duration? In the Abbeville

beds there seems to be clear evidence of four successive stages of submergence, each distinguished from the other by different mineral conditions. The first bed, that in which the bones were entombed along with the human implements, indicates an action strong, if not violent, but not of long duration. The second indicates, by its finer materials, the action of a gentler force. The third seems to be very much a repetition of the first; whilst the last can only be accounted for on the supposition that fine sediment had time to accumulate in comparatively tranquil waters. The interest of the question is very much centred in the nature of the action which began this series of events. Perhaps it may be well to look at the conclusion come to in respect to the origin of the mammaliferous drift-gravel by the geologist who has devoted most special attention to the subject, and before the discoveries of Abbeville had disturbed any preconceived idea. I find Mr Prestwich, in a lecture delivered in 1857, coming to this conclusion in respect to the ossiferous gravels of the Thames:—"Taking into consideration the absence of contemporaneous marine remains, and noting the immense mass of but slightly worn débris derived from and covering irregularly the sedimentary deposits; and the fact that it has evidently been transported from greater or less distances, combined with the occurrence in the gravel of the remains of large land-animals, of trees, and of fresh-water land-shells, we have, I conceive, at all events in these facts, indications of at least one land-surface here destroyed, and its rocks, plants, and animals involved in one common wreck and ruin."

An able and elaborate paper on the "Distribution of the Flint-Drift of the South-east of England," &c., was communicated to the Geological Society of London by Sir R. Murchison in 1851. The phenomena he describes seem everywhere to be a precise repetition of those of Abbeville. Everywhere the flint-drift, which is often, as there, covered by brick earth, clay, or loam, is characterised by the bones of the great extinct mammalia, and everywhere, according to the author's view, gives evidence of sudden and violent diluvial action. Everywhere, also, this drift-gravel rises high above the levels of the existing drainage, whilst, at the same time, it gives evidence that the general configuration of the surface was substantially the same as now. Everywhere, also, wherever shells have been preserved, they belong to our existing fauna, and thus prove

beyond a doubt that, geologically speaking, the age of the drift is the age of the existing world. "In short," he says, "the cliffs of Brighton afford distinct proofs that a period of perfect quiescence and ordinary shore action, very modern in geological parlance, but very ancient as respects history, was followed by oscillations and violent fractures of the crust, producing the tumultuous accumulations to which attention has been drawn."

Unless, then, the Abbeville beds of drift can be separated from those so widely prevalent in other countries, the discovery of human implements underneath this drift will rather tend to bring nearer to us than had ever been supposed some great and sudden diluvial action, than to cast any very clear light on the absolute time—that is, on the time measured by years or centuries—which has elapsed since the creation of our race. The facts which have been brought to light prove, indeed, clearly enough, that since man walked the earth some great changes have affected the condition of its surface; and it is impossible as yet to say what bearing this discovery may be found to have on that remembrance of at least one great catastrophe, which is not more a part of sacred history than it is of profane tradition.

We must not, however, shut our eyes to the indirect effect which this discovery must have on the question of positive time. In the first place, there is a school of geologists, led by our distinguished countryman Sir Charles Lyell, who disbelieve generally in those conclusions which point to violent and sudden changes; and, in the next place, it must be remembered that changes which in point of geological time might well be accounted rapid, might nevertheless well occupy thousands of our years. There is proof in those gravel-beds of the Somme of a double motion, one of submergence to the depth of certainly more than 100 feet, another of subsequent elevation, during which the immense mass of material which had been brought down and deposited by water, has been worn through and broken into escarpments, either by the existing stream or by more powerful currents. We have no data from which to measure in years the time which the accomplishment of such a series of changes may imply. But I think the general impression left upon the mind must be in favour of a very high antiquity. Farther light may be cast upon this subject if the drift-gravels of France, the south of England, and other countries, can be co-ordinated with any one of

the stages of operation to which we owe the superficial deposits of Scotland and the north of Europe generally. It is well known that in these last there is one prominent characteristic which is absent farther south. I mean the abundant proofs of glacial conditions, or an arctic climate. On this subject there is a paper of great interest in the last "Quarterly Journal of the Geological Society," by Mr Jamieson, founded on observations made mainly in the county of Aberdeen. The cycle of changes which this geologist thinks can be clearly traced, as necessary to account for the superficial deposits of our own country, amount to no less than five great epochs, including two of submergence and two of elevation, and involving changes of level to the extent of more than 2000 feet. Scotland has long ago furnished evidence as clear as that founded on the French flint implements, that at least previous to the last of these elevations man had reached her shores, and navigated her rivers and estuaries in those rude canoes, hollowed out of trunks of oak by stone hatchets, which have been frequently found in elevated beds of silt and gravel in the valley of the Clyde. And here we strike upon evidence which has some bearing upon the question of time. Closely connected with the period preceding the last elevation of the land, we have proof that an arctic climate prevailed over a large part of the northern hemisphere, whose climate is now comparatively temperate. But this period seems clearly to have been one of long duration—that is to say, of such duration, and lasting under such conditions of comparative rest, as to allow the development of a glacial fauna. Close to my own residence on the Clyde, each low ebb exposes numerous examples of the *Pecten Islandicus*, and of those very large *Balani*, which are now confined to arctic seas. These beds of shells, which are all of existing species, but of species which have retired from our now more genial temperature to a northern habitat, were first described by my friend Mr Smith of Jordanhill, and his observations and conclusions have since been abundantly confirmed. We have no knowledge how this period was brought to a close. But there seems to be evidence that it had come to an end, and that for a long time before the last elevation of the land, and before man had appeared in Scotland. This seems to be a legitimate deduction from the fact that the canoes in the elevated Clyde beds are formed of oak of large dimensions and of great age. Forests which afforded such timber must have flourished in a climate not much more rigor-

ous than that which exists at present. Here again, then, the earliest footprints of our race are traced up to a point, preceding indeed some important physical changes, but clearly subsequent to the establishment of all the main conditions which now affect the distribution of animal and vegetable life.

As regards the extinction of some animals, I have spoken as if the contemporaneousness of man with them whilst yet living ought not to be absolutely assumed merely from the fact that his implements are associated with their bones. But on this point new evidence is being rapidly collected and brought together. Mons. Lartet, a distinguished French naturalist, has found what he considers to be distinct evidence of the mark of human weapons on various parts of the skeletons of the extinct mammalia of the drift. These marks have been detected on the skull of the *Megaceros Hibernicus*, or great Irish elk,—an animal which stood some ten feet high—on the bones of the *Rhinoceros tichorinus*, and on those of various species of the ox and deer, which are now either extinct or confined to the last remnants of a declining race. The marks are of various kinds—some of them peculiar—indicating a sort of sawing with some instrument not of the smoothest edge. M. Lartet has ascertained that these blows and cuttings could not be made except on fresh bones—that is to say, on bones undried and retaining their animal cartilage. Farther, he has succeeded in producing on the bones of existing animals precisely the same peculiar forms of incision by using one of the old flint implements found in the same beds of gravel, whilst he has equally found that similar marks are incapable of being produced by implements of metallic edge. His conclusion is thus stated by himself:—"If, therefore, the presence of worked flints in the diluvial banks of the Somme, long since brought to light by M. Boucher de Perthes, and more recently confirmed by the rigorous verifications of several of your learned fellow-countrymen, have established the certainty of the existence of man at the time when those erratic deposits were formed, the traces of an *intentional* operation on the bones of the rhinoceros, the aurochs, the megaceros, the cervus sommensis, &c. &c., supply equally the inductive demonstration of the contemporaneousness of those species with the human race."

The great number of flint implements which have been found in the French beds—said to amount to upwards of a thousand in a few

years—when compared with their great rarity elsewhere, is not perhaps so curious as at first sight it may appear to be. Flint implements can only be made where flints are accessible; and it is well known that the flints of particular beds, or strata, of the chalk, are more easily fashioned than others. It is therefore probable that some such favourable locality had existed in the chalk of that part of France, and that what may be called a manufactory of them had consequently been established there. It is remarkable that some of the implements are only half finished, whilst all of them exhibit such sharp edges and angles as are sufficient to prove that they have not been transported far from the spot where they were made, nor subjected to long wear from use.

On the whole, then, it is not to be doubted that the discovery of human implements under repeated beds of aqueous drift and sediment, so high above the levels of existing rivers, or of the existing sea, is a fact of very great significance and importance. In its bearing on geology, it is principally interesting as proving at how recent a period portions at least of the earth have been subject to powerful and rapid diluvial action. In its bearing on human chronology, everything depends on the degree of suddenness and rapidity with which water may have been brought to act upon the former surface. But here anything like data for positive computation entirely fails us. We have no knowledge, in historic times, of any aqueous operation on so grand a scale. Making, however, every deduction which can be made, we must be prepared to find that the facts thus brought to light in the valley of the Somme will be held to furnish important collateral evidence in support of the reasoning founded on other sciences, such as philology and ethnology, which has long demanded, for the development of our race, a number of years far exceeding that which is allowed by the chronology previously received. It is the beautiful expression of Sir Thomas Browne, which I find quoted by Dr Mantell in a former paper on this subject, that "Time conferreth a dignity upon the most trifling thing that resisteth his power;" and it is impossible to look at these rude implements—perhaps the earliest efforts of our race, in the simplest arts of life—without being impressed with the high interest of the questions with which they seem to be inseparably connected.

I think it is impossible not to consider the publication of Mr Darwin's work on the "Origin of Species" as an event in the history of scientific speculation. The influence which such theories have had in stimulating and directing the progress of actual discovery, entitles them, when they come from distinguished men, and when they rest on any large amount of careful observation, to the marked attention of such Societies as this. It cannot be denied that Mr Darwin's book claims our respect on both these grounds. It may be true, as I think it is, that all the facts he has brought together, supposing them to be clearly established (or even much extended by the volume of proof which is still in reserve), bear a very small proportion to the purely speculative conclusions which go to make up his theory on the "Origin of Species." Yet probably there is no other man now living who could have made such a rich collection. No other man since the death of Humboldt has had such powers of observation, combined with such opportunities of observing. "The Voyage of the Beagle" shows how large and wide has been his experience of the general aspects of nature; whilst his monograph on the Cirripedes, and other papers on zoology, testify to his unwearied assiduity in the examination of detail. His book, therefore, comes before the world with every claim to respectful consideration which can be founded on the high scientific reputation of its author. The "Origin of Species," however, means nothing less than the method of creation; and this is a subject so profoundly dark, that no amount of existing knowledge can enable any man to do more than walk carefully round its outer margin, noting where, here and there, some fact, more significant than others, seems to give hope of entrance into the obscurity within. The particular theory advanced by Mr Darwin is but a special form of the old theory of development; special in this respect, that it professes to point out the particular law under which every animal and vegetable form may have been derived from those pre-existing, by ordinary generation. One general admission may, I think, be safely made in reference to all such theories. They are undoubtedly more easily conceived than what is called "creation." But this is not saying much. The truth is, that creation, of which we often talk so easily, is a work of which we have no knowledge and can have no conception. Something is known of the laws under which organic beings, once created, are enabled to continue their existence and to propagate their kind; and it is, of course,

comparatively easy for us to conceive some such modification of those laws as Mr Darwin suggests,—to suppose that any given animal should occasionally produce offspring slightly different from itself in some one portion of its structure, and that such differences should go on accumulating, until finally they end in the most divergent forms. But to imagine processes which shall be the most easily conceivable goes but a very little way in science; and, after all, the difficulty is but postponed. Mr Darwin himself is obliged to have recourse at last to the ordinary forms of language in which the idea of creation is expressed, and speaks of a primordial form into which "life was first breathed." In science we may sometimes allow the question to be asked, "What is most easily conceivable?" but only on condition that it be followed hard by the farther question, "How much of this easiness of conception is gained at the expense of departure from the evidence of facts and the experience of nature?" In answer to this inquiry, it may well be doubted whether Mr Darwin has proved one single fact capable of sustaining the very first step in his ingenious argument. That argument seems to be as follows: Man has succeeded by "artificial selection"—that is, by careful "breeding"—in establishing certain modifications in the forms of domestic animals. Therefore, similar results may be produced to an infinitely greater degree by nature. Only, the principle of selection will be different. Man chooses those qualities which are most useful to him as master. Nature will choose those which are most useful to the animal itself. But the qualities which are most useful to an animal will be those which enable it to survive when its fellows and congeners die. If, therefore, any such qualities arise in any particular family or breed, they will be preserved and perpetuated. This is a beautiful theory. But when we ask how far the facts carry us towards the "origin of new species," we find that there is in reality no perceptible advance. The changes producible by breeding, or by "artificial selection," are all confined within a circle which indicates a restraining law. The changes producible by "natural selection" are, so far as we know and can observe, under similar, if not under still narrower limitation. As regards the first, Mr Darwin himself supplies us with an illustration beyond all others striking, of that law of reversion to type, the existence of which he nevertheless disputes. Pigeons are his favourite example of extreme modification of form. They have been "bred"

or selected for three thousand years. Mr Darwin took two of the unlikest and most aberrant parents he could select, a black "barb" and a white "fantail." The result was, that a grandchild of these parents exhibited a close return to the old primal type, the rock-pigeon, from which all domestic pigeons originally sprung. Yet who knows through how many generations of "selected parents"—perhaps from the days of the Pharaohs—this chick had inherited its ancestral colours! Can there be a stronger illustration of that restraining law of reversion to type, which, so far as we know, confines within a very narrow circle, not only the extent, but the duration of aberrant forms of life? Then, as regards natural selection, do we know of any one authentic instance in which new conditions of life have been met by such modifications of structure as might enable an animal to survive its congeners in the "battle of life?" Our experience in this way is perhaps fully more extensive than in any other. The truth is, that man is himself the greatest modifier of the natural conditions under which the lower animals are placed. He is year by year producing revolutions which might be equivalent to centuries of natural change. Nor are these without a powerful effect on animal life. Mr Darwin has traced the changes thus produced with singular ingenuity and beauty of description. But all those changes are produced by the substitution of one species for another,—never by the modification of the same species to the new conditions which surround it. There need be no dispute that, under the law so beautifully traced by Darwin, such modifications, *if they did arise*, would tend to survive and be perpetuated. But what we want is—facts to justify the supposition that any such modifications do actually arise; such, for example, as would enable an animal adapted for marshy land to survive on land which had become dry;—or arboreal forms to survive the destruction of their native forests;—or land-animals to adapt themselves to a country which is being gradually submerged. These are all operations of which man has had experience, and to some of which he is every day contributing; yet no instance is recorded of nature having ever had any opportunity of exercising in favour of any animal that "selecting" power which is the assumed origin of new species. The Fauna is indeed changed by such changes of condition as I have supposed. But that change is effected by substitution, not by conversion. One animal or plant in-

vades the former territory of another. In our own country, for example, the grouse gives way to the partridge, or the snipe to the landrail; or, more rarely, the lark may be supplanted by the waders and the gulls—the field-mouse and the mole by the water-rat and the otter. But in no case that we know of, or that Mr Darwin has adduced, has any wild animal been enabled, by any modification of form, however slight, to survive any essential changes in that condition for which it was first adapted. And as this is the law which obtains in the present, so also it is the law which appears to have obtained in the past. The absence of any evidence of the passage of one form into another, discoverable in the records of former worlds, is confessed by our author himself. All his arguments are directed, not to deny this fact, but to explain it. It has been truly said, in a very able and interesting paper on the subject which was communicated to this Society by one of its members early in the present year, that "The strongest points in favour of the general results come to by Mr Darwin, are a class of facts which can scarcely be said to bear distinctively on his theory more than upon various other theories already promulgated, and more or less adopted. One of these is the fact that all animals and plants, throughout all time and space, should be related to each other in group subordinate to group, ... another not less formidable fact is the existence of the same homological parts in different animals, sometimes aborted, and sometimes largely developed." The endeavour to explain and account for these strange connections and relationships is one of the highest aims of science. To refer them to the great law of hereditary descent is a very natural suggestion, and for a moment some minds may be disposed to rest in it as a kind of explanation. Reduction to a known and familiar law is the nearest approach to explanation which science can afford. But we must beware of the subtle error which lies in changing a law well known and familiar, into another law entirely unknown and new, by ascribing to it effects and operations of which we have no experience. If the law of descent by ordinary generation is consistent with the origin, through this means, of new species, some proof must be given of the fact. Until such proof is adduced, the assumed law is not that of *ordinary* generation, but of *extraordinary*—of a new kind of generation essentially different from that of which we have any knowledge.

It is well worthy of remark, that Mr Darwin holds strongly to the

doctrine of "single centres of creation"—or as, to suit his special theory, they ought perhaps to be called, single centres of birth. He believes that each new species came into being at some one spot only, and that, however wide may be its distribution now, such distribution has been due wholly to dispersion. "If the same species," he says, "*can* be produced at two separate points, why do we not find a single mammal common to Europe, Australia, and South America? The conditions of life are nearly the same." But surely this belief in single centres of creation or of birth is not very easily reconcilable with the rest of Mr Darwin's theory. The essential idea of that theory is, that new species arise from any accidental variety which enables the animal possessing it to have some special advantage in the struggle for existence. But, as similar modifications of structure would in this respect confer similar advantages, at one time or other, under some circumstances or other all over the globe, it is impossible to understand why they should not frequently arise at many different points, either at once, or in succession. We may freely grant, therefore, to Mr Darwin that his reasoning explains to us how a given species, *once born*, and which begins the battle of life under favourable conditions, should rapidly spread, and should extinguish its congeners and predecessors, which are less favourably endowed. But it gives us no sort of explanation, or even suggestion, of the law *under which any such new species is first produced.* How such a new birth comes to be determined, and above all how it can only be determined at some one spot of all the million spots on which the same parents flourish, remains as profound a mystery as before; and we have in reality not advanced a single step towards the "origin of species."

The conclusions arrived at by Mr Darwin are essentially but another form of the old theory of development, and as such they will meet with the same vigorous resistance. We may cordially join in the warning of Professor Huxley, that the arguments of such a naturalist as Mr Darwin must be met on scientific grounds alone. And yet the difficulty, to use no stronger word, of reconciling this theory when applied to man, with all that we know of his physical and moral nature, and all that we have hitherto believed respecting his early history, is at least one among the many difficulties which may well call for the most jealous and critical analysis of every step in Mr Darwin's argument. He himself, indeed, seems

to feel no difficulty in the matter—lineal descent from some early fish or reptile—"some ancient prototype furnished with a floating apparatus or swimming-bladder"—Mr Darwin regards as the noblest claim of ancestry. "When I view all beings," he says, "not as special creations, but as the lineal descendants of some few beings who lived long before the first bed of the Silurian system was deposited, they seem to me to be ennobled." I am afraid that the honour of this parentage, as regards our own species, will not be universally appreciated. The question, however, is not whether it be "ennobling" or the reverse, but whether it can be proved or rendered in any degree probable. Yet, in judging of the sufficiency of evidence, it is well to recollect the full weight of the conclusion which that evidence must be strong enough to bear; nor, in this point of view, do I think it wholly unphilosophical to bear in mind the innate beliefs and instincts of mankind.

It is not, however, my duty or my desire, in this place and on this occasion, to enter more deeply into the specific argument on the "origin of species;" I would rather indicate wherein the discussion, and the argument which has raised that discussion, has most directly tended to the advance of science. In this respect, it is not too much to say that the whole book is full of the most curious and original observation, and exhibits in an eminent degree that power and habit of arranging and co-ordinating physical phenomena which is essential to the attainment of great results, and which it has been the special use of such theories in the history of science to evoke and to direct. In particular, I think no one can read Darwin's chapter on the "struggle for existence," or the two chapters on "geographical distribution," without feeling that new and important light has been cast on subjects which are as interesting as they are difficult and obscure.

I hope I need not assure the Members of this Society how highly I value the honour which places me in this chair. To be chosen President of a Society of which the two former Presidents were Sir W. Scott and Sir T. Brisbane is indeed an honour of which any Scotchman may well be proud. But whilst these names are of themselves sufficient to indicate how great that honour is, they are not less sufficient to remind me that your choice of President is determined on different occasions by considerations of very various

nature. Sir W. Scott's unbounded literary fame, and the personal affection in which he was held in Edinburgh, placed him in the front of all men who could be competitors for the chair of a Society of which already he was the most illustrious member. Sir T. Brisbane was not only one of the most renowned soldiers of his day, but was besides a man of high scientific attainment, and a promoter of science as wise as he was munificent. I can draw from the choice which you have lately made of a successor to such distinguished men no other inference than that this Society places a very large and generous interpretation upon the qualifications requisite in its President,—that you are willing occasionally to connect the office with those pursuits of public life which, whilst they are unfavourable, I am afraid, to any sustained scientific inquiry, are not incompatible with a sincere interest in the progress of science, and a high appreciation of its value to mankind.

The following Donations to the Library were announced :—

Proceedings of the Royal Society of London. Vol. X., No. 38. 8vo.—*From the Society.*

Proceedings of Berwickshire Naturalist's Club. 1859.—*From the Club.*

Résumé Météorologique de l'Année 1858, pour Genève et le Grand St Bernard. Par E. Plantamour. Geneva, 1859.—*From the Author.*

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Additional Notes on the Post-Pliocene Deposits of the St Lawrence Valley. By J. W. Dawson, LL.D.—*From the Author.*

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On the Silurian and Devonian Rocks of Nova Scotia. By J. W. Dawson, LL.D.—*From the same.*

On Fossil Plants from the Devonian Rocks of Canada. By J. W. Dawson, LL.D.—*From the same.*

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